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What is syntactic about reported speech/discourse?

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Spronck and Nikitina's paper is an attempt to locate the domain of reported speech (aka reported discourse, henceforth RD) within language from a cross-linguistic perspective. RD has been notoriously problematic to define and relate to other phenomena; so in spite of it having been extensively studied in the recent and not-so-recent past it still deserves close attention. Given the experience of the authors in this field and their extensive referencing of important contributions,¹ one could expect the article to provide a typologically oriented state-of-the-art report as well as advance the discussion. Unfortunately, however, my expectations are met only to a limited extent.

As stated at the beginning, the authors set about “defending” the central claim (without clarifying whether they have a counterclaim in mind) that RD

constitutes a dedicated syntactic domain, i.e. crosslinguistically it involves a number of specific/characteristic phenomena that cannot be derived from the involvement of other syntactic structures in reported speech, such as subordination.

This introductory statement, like the title of the article, implies that the authors want to tackle first of all the *syntax* of RD. In reality, however, large portions of the article and, for that matter, its major strength, concern the discussion of semantic and pragmatic issues of RD. Here, the linguistic facts, already well known from the previous literature, are discussed insightfully and are likely to prompt little opposition. However, based on the central claim above, I expected arguments that RD can be/is:

¹ There are nevertheless notable cases of selective and biased referencing of and/or engagement with the previous literature. One wonders, for example, why only Evans (2013) is mentioned in connection with the scalarity of RD in terms of perspectivization, as this approach has been taken at least as early as Roncador (1988). While this is admittedly an oversight by Evans, Roncador's important contribution to the discussion of RD in general should have been acknowledged by the authors, not the least because Roncador (1992) also advanced the study of logophoric markers - another crucial topic for their discussion. Other instances of an unbalanced treatment of previous relevant work are mentioned below.

- (a) established as a domain on account of traits that are (predominantly) syntactic,
- (b) shown to be internally unitary in syntactic terms,
- (c) sufficiently delimited from other syntactic domains, and
- (d) plausibly a universal syntactic category.

Regarding none of these points does the paper make a strong case. Accordingly, I largely restrict myself in the following comments to the syntactic profile of RD as the seemingly central problem of the article. I do not deal with other non-structural issues of the status of RD that would also deserve discussion in a comprehensive treatment (cf., for example, the relation of direct RD to what Güldemann (2008, chapter 4) identifies as the linguistic domain of “mimesis”).

In § 2, the crucial identification of RD as a “dedicated syntactic domain” is argued to be justified by “eight cross-linguistic features that ... characterize reported speech in the languages of the world and set it apart from other syntactic categories.” These concern:

1. the syntactic relation of M(atrix) and R(eported) clause in binary RD expressions (§ 2.1),
2. the structural variability of RD expressions, including the absence of M (§ 2.2),
3. the different semantic and pragmatic interpretation of “indexicals” in M and R (§ 2.3),
4. the occurrence of “indexicals” that are unique to R (§ 2.4),
5. the modal shift between M and R involving “bi-perspectivization” (§ 2.5),²
6. the multiple and scalar perspectivization shift with predictable semantic effects (§ 2.6),
7. the change of RD into other functions along an evidential-modal-aspectual cline (§ 2.7)
8. the “multimodal” nature of RD involving in particular gesture and prosody (§ 2.8)

I will hardly be alone in having no reason to argue much about any of these eight features. Crucially, however, one wonders how they can motivate the central claim of the article. The majority of the features, i.e. the five in §§ 2.3–2.7, are semantic-pragmatic rather than syntactic in nature. Only the remaining three relate to syntax, at least to some extent, although the second in § 2.2 and the

² For the record, bi-perspectivization of the purported original text and the version produced by the reporter is the principal meaning of “distancing” in the definition of RD by Güldemann (2008).

last in § 2.8 are in fact about the absence of syntactic coding means. Only the first trait in § 2.1, referring to the binary RD structure involving M and R, relates directly to syntax but is clearly not universally relevant for the domain.

The difficulty in justifying a purported “cross-linguistic syntactic category” by means of semantics becomes yet clearer in connection with the proposed new definition of RD in terms of “three *meaning* components”. As a consequence, a sentence like *I am telling you that he is in for a surprise* in (34), for example, is excluded from RD on account of its interpretation and non-compliance with the authors’ semantic definition, although there are obviously no formal grounds to dissociate it from canonical RD expressions. Conversely, there is certainly no unifying structural profile in the examples of (32’)-(32’’) but they are all viewed as instances of a single “syntactic domain.” It is possible that the issue of syntax as opposed to semantics is contingent on the authors’ stance regarding syntactic theory, which, however, is nowhere spelled out.

As none of the RD features of the authors that are plausibly syntactic are definitional in terms of their central claim, is there any unifying syntactic feature of RD as a whole? One may be tempted to venture a positive answer, namely the fact that this single domain displays such highly diverse structures as crucial formal options. I refer in particular to the opposition between *simplex* R clauses that display a linguistically overt shift of perspective (possibly accompanied by marked prosody and non-verbal gesture) and the different types of *complex* M + R structures that incorporate such simple shifted R expressions. If anything, a more abstract syntactic trait of RD is its very degree of structural *diversity*, as opposed to other arguably more coherent syntactic domains. This needs to be accounted for in an article that deals with RD as a “universal syntactic category.” Although the authors recognize that “expressions of reported speech display great diversity,” the wide constructional range of RD constructions is hardly discussed and thus remains underexposed (see Güldemann 2008 for an extensive cross-linguistic treatment and some typological generalizations).

Regarding the attempt to identify a unitary syntactic profile of RD, one also wonders why the authors engage so little with previous relevant opinions. They cite D’Arcy (2015), who “goes even so far as to deny syntax any special privilege in the description of reported speech”, but do not argue for their own apparently opposite stance. Equally surprising is that, although admitting in a footnote to being “heavily indebted to the analyses in Güldemann (2008), and McGregor (1994),” they hardly deal with the new syntactic proposals there. These two works attempt to reconcile the apparently problematic coexistence of two quite distinct structures in the RD domain, namely on the one hand binary M + R expressions that for a long time have motivated some syntactic analysis

in terms of traditional clause linkage, and on the other hand plain R expressions that lead a generalized clause-linkage account ad absurdum.

McGregor (1994: 77) compares R and M with a “whole-whole relationship between a picture and its frame” and thus sets it off from traditional clause-linkage analyses:

The picture, its frame, and the framed picture may be regarded as distinct wholes, none of which is in any significant sense a part of some larger whole. ... The picture and the frame are entities of very different characters: the picture represents some referent world - it is an icon. The frame clearly is neither an icon, nor does it represent something in the referent world. What it does is set the icon off from the context ... In doing this it also provides information as to how the icon is to be viewed ...

Spronck and Nikitina mention the “framing relation” model briefly in connection with binary RD structures without much explanation, merely re-labeling it in Spronck (2017) and the article at issue by such terms of their own making as “frame-in” and “defenestration” (the second relating to the important “frame”-less RD option involving an R without an M).

The non-traditional syntactic proposal by Güldemann (2008: 232–3) is ignored entirely by the authors. My idea of M being an optional “tag” on R incorporates a lot of McGregor’s “picture-frame” model but tackles a few arguably critical points of his account. My reasons for not simply taking over McGregor’s concept are threefold. Purely in terms of terminology, I prefer to avoid the term “frame,” because it evokes the structural aspect of M flanking R on both sides, which is possible but certainly not a general phenomenon. My two other reasons are more substantial. McGregor states that none of the three linguistic entities, namely R, M, and the entire RD structure, “is in any significant sense a part of some larger whole.” I would claim, however, that once an R is accompanied by an M the resulting structure can indeed be fruitfully analyzed as a “larger whole”, namely a binary RD construction. Finally, it is also not feasible to simply view an M as an independent “whole.” Some properties of M indicate clearly that it is, as opposed to R, somewhat incomplete. A phrase conveying something like *X say/think* obviously induces the presence of a reported text. This effect is also ingrained structurally, for example, in the many instances of M containing a semantically superfluous element that explicitly refers to R, called variably “quotative”, “complementizer” etc. and subsumed in Güldemann (2008) under the general label “quote orienter.” This one-sided incompleteness of a plain M captures the observation that an RD construction can lack an M but not an R. An M without an R is like a dog collar without a dog or, in McGregor’s terms, a frame without a picture. This directly relates to the terminological metaphor of a “tag” proposed in my earlier account:

a tag requires or at least evokes an entity with which it is associated. At the same time, it implies that the authors' "M(atrix)" and similar technical terms are misnomers; my neutral terminological proposal is "quotative index."

Both McGregor's "frame" model and my "tag" model fall outside the range of traditional clause linkage types; but they can explain properties of RD constructions that are problematic for both the complementation and the parataxis approach entertained previously. They account for the syntactic autonomy of R, and they explain the syntactic ambiguity of verbs that can be canonically transitive with respect to true nominal entities but behave intransitively vis-à-vis R (cf., e.g. Munro 1982). The considerable variability in the placement of M(-segment)s before, within, and/or after R is compatible in particular with my "tag" model: where to tag an entity, and possibly how often, depends on the entity itself and the context of its use. The model also motivates the above-mentioned impression that an M without an R is incomplete, yet without invoking this as evidence for transitivity. A historical dimension compatible with the "tag" model is that Ms frequently develop into grams like clitics or affixes that are bound to (former) Rs. This can be conceived of as an extreme conventionalization of tagging. Finally, this approach naturally accounts for M-less RD constructions. A tag is in principle a dispensable appendix. Its absence can certainly make the identification of a conventionally tagged item more difficult, but it does not render it completely impossible, because the latter maintains its own character, just as a suitcase without a tag remains a suitcase. In Güldemann (2008, § 3.4.2) I also argue that the "tag" model not only accounts for direct but also a large portion of indirect RD, so that the entire domain should by default be syntactically separated from instances of canonical clause linkage, in particular from truly subordinating sentential complementation.

My reference to subordination and the like serves as a welcome transition to mentioning another open question in the argument of RD as a dedicated syntactic domain. That is, if not obviously unitary internally, can it at least be delimited from other linguistic domains? The programmatic title of Güldemann and Roncador's (2002) edited volume, which characterizes RD as "a meeting ground for different linguistic domains," suggests that there is and has been a lot of discussion about RD fading into other expression types in various ways, both formally and functionally. The problem of the article under discussion is that not only is there no attempt to delimit an assumed "syntactic RD domain", the potential "neighboring" domains are not even clearly and fully identified. Thus, on the basis of the discussion, the synchronic and/or diachronic relation of RD expressions to similar yet different constructions remains all but clear, and

there is no answer to the question which borderline cases do not (or no longer) belong to RD as a syntactic phenomenon.

A final issue is the invoked universality of RD in a syntactic sense. This reflects a problem that pervades many typologically intended treatments of grammatical phenomena. I do not refer to the fact that the article lacks any exposition of representative cross-linguistic data justifying the authors' generalization(s) - arguably, this is due to the publication context. My observation relates to what the authors briefly note themselves, namely that the "decision whether the examples introduced [...] constitute reported speech *constructions* ultimately is a specific morpho-syntactic question about English [or any other language]." In line with, for example, Haspelmath (2010), I fully subscribe to this view for the assessment of any language-specific structure and its status vis-à-vis a certain *syntactic* domain, and hence for the profile of such a domain itself, both in a single language and in general.

Overall, I cannot agree with the authors' conclusion that their "observations ... strongly suggest that reported speech constitutes a syntactic class in its own right." Moreover, their attempted contribution to constructional typology requires a different approach. This would start with establishing RD as a comparative concept on primarily semantic-functional grounds, something the article certainly contributes to, but then has to really look at graspable syntax in order to see whether or how this comparative concept maps over types of linguistic form, first in individual languages and then in cross-linguistically more regular patterns.

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